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NOTES AND LITERATURE

COMPARATIVE PSYCHOLOGY

Bohn's "The Birth of Intelligence."[1]—The good critic, says Anatole France, is he who recounts the adventures of his soul among masterpieces. If the reviewer recounts his adventures with Dr. Bohn's fascinating book, he can perhaps give a better idea of it than in any other way. Dr. Bohn has for years been engaged in a study of the behavior of the animals of the seashore, as influenced by the conditions under which they live. His published papers set forth that the behavior of these animals—the worms, crabs, snails, sea anemones and the like—is wonderfully dependent on the past influences they have undergone; the changes of tides, of day and night, and the like, leave their records on the animals, so that after removal from these agents, the creatures still show the habits formed under their influence. Thus the behavior of these creatures is not fixed and final, but surprisingly modifiable. Such complete and overwhelming confirmation of the results and views that had been independently expressed by the present reviewer and going even beyond what he had anticipated, could not be received by him otherwise than with enthusiasm; he recommended the work to his friends and wrote a laudatory review;[2] as Bohn in the present volume notes with satisfaction, his work "surprised the Americans" (p. 71). But the reviewer's enthusiasm was met in some quarters by skepticism and criticism; it was pointed out that Bohn's results are largely given *en masse*, as it were, and with a striking lack of that precision of detail and of method which most of us have felt requisite for establishing scientific results; Yerkes in 1906 said, in a review of his papers, "They are not thoroughly satisfactory scientifically for they continually suggest questions, doubts and new problems."[3] The work was of precisely the kind, some urged, in which inaccuracy, carelessness or prejudice would enable one to reach results seeming to confirm whatever theory the author wished to establish.

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[1] Bohn, Georges, " La Naissance de l'Intelligence," Paris, 1909.

[2] *Psychological Bulletin*, 5, 1908, pp. 180–183.

[3] *Journ. Comp. Neurol. and Psychol.*, 16, 1906, p. 238.

It was, therefore, with special interest that the reviewer greeted the appearance of a general work by Bohn; on the one hand, it would naturally deal with phenomena that the reviewer is convinced are of special importance; on the other, it was hoped that the care and accuracy of presentation would be such as to remove all doubts as to the validity of Bohn's scientific results.

As to the first point, a general examination seemed to show one's best hopes gratified. The book might be called an amplification of the text—"The reactions of a living creature at a given moment depend not only on the present conditions, but on all the conditions of the past life—not only the life of the individual under consideration, but also that of its ancestors" (p. 259).[4] This is the spirit in which I have desired to see written a book on behavior.

The book is not put in the form of a connected account, but is rather a series of discussions of general topics and problems, illustrated by examples. Discussion of the work and views of other investigators occupies a large space; here the author paints in large, sweeping strokes, pronouncing as to good or ill with absolute confidence. This method makes lively reading, but it requires great and accurate knowledge of details and a most fair and judicious spirit to use it and remain scientific; without these qualities it is cheap, easy and misleading. The author's qualifications in these respects we shall inquire into later.

Many of the author's general principles and methods seem excellent; he attempts to distinguish the varied different factors in behavior; to establish definite meanings for terms long under doubt or controversy (so for example the term tropism); to search for objective factors everywhere; he recognizes fully the complexity and modifiability of behavior. The reviewer finds especially sympathetic the section on the uselessness of the idea of *instinct* as an analytic concept; the emphasis on the fact that all adaptation or "finality" is a problem, not a solution (p. 286); the idea of the "struggle against variation" (Ch. 15), and indeed many other matters. Even where he would disagree, the discussion is interesting and often valuable.

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[4] This sentence is used by Bohn as a summing up of ideas expressed by Semon; it seems to me however to express the idea of this book, though here Bohn possibly might not agree.

Little reading of the book is necessary to show, what has been evident from recent papers, that the author is a hero worshipper, and that at present the hero of his tale is (most deservedly in a book on the objective study of behavior) Jacques Loeb. Yerkes is given a place as a worthy follower of the master, while the present reviewer takes the rôle of the villain in the plot. It will be worth while to translate Bohn's characterization of three American investigators, as giving an example of his style of thought and expression. I quote from an article in the *Revue Scientifique* of May 16, 1908, which gives some picturesque features not taken into the book.

> I have pronounced the name of Galileo; I can not resist the temptation to inscribe by the side of this name that of the biologist JACQUES LOEB.
>
> Certainly since Galileo times have changed. No longer are revolutionary savants persecuted, at least not openly. There are free countries such as America for men such as Jacques Loeb. Beyond the Rocky Mountains, on the shores of the most beautiful bay in the world, at Berkeley, opposite San Francisco, are the laboratories whither the Californians have called the great biologist; it is in a scene truly fairy-like that Loeb works without ceasing and whence he sends forth the ideas which pass through the world to give a significance to biological researches.[5]
>
> However, in America they continued to gather facts. And a savant of great worth, YERKES, completed without noise the work of Loeb[6] by searching out, with a spirit of method truly remarkable, how associations are formed in animals; his two memoirs on the acquisition of habits in the frog and the crayfish will remain classic and must serve as models to all those who attack the same questions. Yerkes has shown a quality that is rare among psychologists; that of not attempting to choose a personal attitude and to attach his name to a system; he has endeavored constantly to establish facts with all the rigor desirable and to give to them wise interpretations.
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> Suddenly[7] JENNINGS appeared on the scene; he presented a system;

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[5] This passage is found also on page 42 of "La Naissance de l'Intelligence."

[6] The precise reference here seems to be to "the phenomena of association studied by Loeb in the lowest animals," words which he has employed a little before.

[7] The fact is of course that I had been publishing papers on this line of work for a number of years before Yerkes began; and that I suggested

that of trial and error, with disciples already numerous, he undertook a crusade against what he called the "orthodox theory of tropisms"; he cast trouble into the spirits, and certain ones could believe for an instant that the work of Loeb was about to crumble. It remained entire. The hurricane which had made such ravages in America made itself felt even in Europe, where it had little trouble to demolish the frail constructions of M. Nuel.[8]

The part sketched for the third actor in the plot is certainly an unworthy one, but there is after all something dramatic about figuring one's self, after Bohn, as a sort of demon that rides upon the storm, carrying trouble to the poor creatures of earth; so this, with the pleasure of seeing the ideas of the complexity and modifiability of behavior in lower organisms for which I have long worked, so completely triumphant in Bohn's book, must perforce be my consolation. It is the traditional fate of innovators to suffer condemnation that their work may prevail.

But there remains the second point to consider; in the book the author gives us a study not only of phenomena at the seashore, which we can not test for ourselves, but of the work and views of other investigators, which we can ourselves examine. Here is a test for the accuracy, thoroughness and conscientiousness of his work, for his mental grasp and his freedom from prejudice—a test for which we have above mentioned the need. Does he stand this test in a way to set at rest the doubts that have been expressed as to the trustworthiness of his scientific results in difficult fields?

For this test I naturally examined his presentation of my own work, since that is the matter with which I am best acquainted; furthermore I was of course curious to see why he presents me as one of the powers of darkness, while I have welcomed him as a support and ally. And here to my extreme disappointment I found things which I think may again "surprise the Americans" and every one else that has any ideals as to scientific accuracy. It would certainly be difficult for any author to

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the application of the idea of trial and error and criticized the theory of tropisms only after seven years of constant work, during which time I had published many papers on the subject. It is more dramatic to bring me in "tout à coup" and the shackles of chronology or other mere facts are not allowed by Dr. Bohn to interfere with effective presentation, as we shall see later.

[8] Le passé et l'avenir de la psychologie comparée," *Revue Scientifique* (50), 9, p. 624.

present a more thoroughly incorrect account of another's work, both as to material facts and as to general views and principles, than Bohn has given of my own. A citation of some typical examples will enable the reader to form an idea of the author's practise and ideals of accuracy.

Any one acquainted with my work on the reactions of lower organisms will recall that I found and set forth, with what seemed to many wearisome repetition, that the cause of reaction is in a large proportion of cases a *change* in external conditions, variation in concentration of chemicals, in degree of temperature, intensity of illumination, etc. Now, Bohn has likewise observed this (as had many previous workers), and he makes large use of it in explaining behavior; under the name of ''sensibilité différentielle'' it forms one of his three main factors in behavior. He is even inclined to lay claim to a certain originality in this.[9]

Amid the ruins [of other men's ideas] the reader, I hope, will see already some new constructions arising. Certain chapters, those consecrated to *vital rhythms* and to *sensibility to differences* figure for the first time in a book on comparative psychology (p. 3).

For this purpose it is convenient that he feels prepared to assert *that I had never recognized this as a cause of reaction!*

> The idea of sensibility to differences has escaped Jennings.[10] In general, Jennings has not carried far enough the analysis of the movements of the lower organisms; as he has not taken account of the phenomena of sensibility to differences, he has been led to speak of "trial and error."[11]

Such statements appear again and again throughout the book, and form the basis for repeated condemnations of my work. It would be difficult to make statements more completely contrary to the facts. As this is the source of most of Bohn's mistakes in regard to my work, it is well to consider the matter in some detail.

No doubt is left as to what is meant by ''sensibilité différentielle''; it is discussed in full with numerous examples; it is the same as Loeb's ''Unterschiedsempfindlichkeit'' and signifies reactions to changes in the environmental conditions. ''Il s'agit, en effet, de réponses à des variations plus on moins brusques des diverses forces du milieu extérieur'' (p. 167).

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[9] Though he recognizes that Loeb had made use of the same idea.

[10] '' La notion de sensibilité différentielle a échappé à Jennings,'' p. 179.

[11] '' D'une façon générale, Jennings n'a pas poussé assez loin l'analyse des mouvements des organismes inférieurs; comme il n'a pas tenu compte des phénomènes de sensibilité différentielle, il a été amené à parler d'essais et erreurs,'' (p. 191).

Consider in connection with the above statements from Bohn, the following: In the table of contents of my book on the ''Behavior of the Lower Organisms'' (1906), we find the caption Change of Conditions as a Cause of Reaction. Turning to the indicated chapter (XVII), as any one would naturally do who desired to form a conception of my results on this matter, we find that the second numbered paragraph begins:

> The most general cause of a reaction is a change in the conditions affecting the organism. This has been illustrated in detail in the descriptive portions of the present work (p. 293).

Practically the entire chapter is taken up with an analysis and discussion of variations in the environmental agents as causes of reaction. In the descriptive portions of the work the point is set forth most explicitly again and again. Taking the account of Paramecium as a type, we find the following:

> It is clear that the cause of reaction is the *change* from one solution or temperature to another (p. 51). The animal, having been subjected to certain conditions, becomes now subjected to others, and it *is the transition from one state to another that is the cause of reaction.* This is a fact of fundamental significance for understanding the behavior of lower organisms[12] (p. 52). But as its movements carry the animal from one region to another, the environmental conditions affecting it are of course changed, and some of these changes in condition act as stimuli, causing the animal to change its movements (p. 58). In the reactions to mechanical stimuli, chemicals, osmotic pressure, heat and cold, and powerful light, the avoiding reaction is caused by the transition from one external condition to another, by change in the intensity of action of some agent (p. 78). Examination has shown us that the cause for this reaction is some change in the conditions (p. 108).

For other organisms the point is likewise developed in detail and stated with equal explicitness. Thus, for Amœba: ''The cause of a reaction—that is, of a change in movement—is in most cases some change in the environment'' (p. 19); for Bacteria: ''The reaction is caused as a rule by a change in the environment of the organism'' (p. 37); for the reactions of Stentor to light: ''all together, then, our experiments have thus far shown that the cause of the avoiding reaction is the change from darkness to light'' (p. 131); for Euglena: ''It is clear that the reaction is due to the decrease in the intensity of light'' (p. 136); for reactions of infusoria in general to chemicals: ''The reaction is in each case caused by a change from one concentration to another'' (p. 123); for their reactions to light: ''To sum up, we find that the reactions to light occur in the infusoria in essentially the same way as do the reactions to most other stimuli. . . . The cause of reaction is a change in the intensity of light'' (p. 149). Pages could be filled with quotations from my book to the same effect.

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[12] The italics are in the original.

In my earlier papers the matter is dealt with in an equally full and explicit way. My paper on ''Reactions to Light in Ciliates and Flagellates[13] is mainly devoted to showing in detail that the reactions are due to variations in the intensity of light. A section headed ''Nature of Agent causing the Reaction'' begins ''The primary and essential cause of reaction is a change of illumination'' (p. 65). My paper on ''The Behavior of Paramecium''[14] has a section headed Nature of Stimulation, in which we find the following:

> An examination of the facts shows that as a general rule the effective stimuli consist of some *change* in the conditions (p. 464).

These quotations are not isolated; they are mere typical sentences in an extended elaboration of the point set forth, and they could be multiplied almost indefinitely.

How then Bohn could make the statement that I had not taken account of this matter is inexplicable. But assuming that I was unacquainted with the fact that forms in reality the foundation of my presentation of the causes of reaction, the author of course cannot possibly understand my account of behavior; it is to him necessarily an edifice without foundations and must therefore crumble to earth. This is the source from which all of his misconceptions and (unintentional) misrepresentations of my work flow as naturally as water from a spring. The chief errors thus arising are the following.

1. He is necessarily utterly unable to understand my concept of ''trial and error.''

2. He is entirely prevented from grasping the nature of my criticisms of the tropism idea.

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[13] Carnegie Institution, Publ. 16, 1904, pp. 29–71.

[14] *Journ. Comp. Neurol. Psychol.*, 14, 1904, pp. 441–510.

3. He is led to suppose that my account, being wrong in principle, must also be wrong in details, so that he attempts to correct details, by giving usually the same explanation that I had already given; or at times some other causal explanation that he devises, without experiment, to account for the facts; this often leads him into curious errors.

4. He is inevitably driven to suppose that I reject determinism in behavior.

5. He is naturally led (though here without real basis as it seems to me, save by additional error of the same sort) to attribute to me psychic, anthropomorphic and finalistic explanations in place of causal ones.

Let us look briefly at certain of these points.

1. My concept "trial and error" the author is forced by his mistake to suppose designed to take the place of an experimental analysis based on a real knowledge of the causes of reaction. His account of its origin is:

> As he has not taken into account the phenomena of sensibility to differences, he has been led to speak of trial and error (p. 191).

The fact is that the idea of trial and error was based precisely on reactions due to "sensibility to differences"; to changes in the conditions, and I have again and again stated that fact. For example:

> Just what is the nature of the stimulation which produces this reaction by "trial and error" in Paramecium? An examination of the facts shows that as a general rule the effective stimuli consist of some *change* in the conditions.[15]

The purpose of the concept was to bring under a unified point of view a complex of stimulations and reactions which had the interesting and important result of bringing the organism into conditions adapted to it, yet not including "anything differing in essential principle from such methods of action as we find in the inorganic world."[16] The concept is useful only in attacking the problem of how adaptation is brought about without the operation of "final causes." To speak of "trial and error" of course does not get rid of the necessity of finding a determining cause for every phase of the reaction, any more than to call a reaction "tropism" has this result. It has been precisely my endeavor to show just how each phase was determined so as to show that "there is no evidence that a final aim is guiding the organism."[17] The application of the phrase "trial and error" to lower organisms arose as follows: The organisms react to changes in the conditions by movements of a peculiar character, which subject them to various environmental changes. Some of these changes cause them to react further, still further changing the conditions. Finally, as a rule, their continued movements bring them into conditions which do not cause them to react by further movements; since they do not react further they remain in these conditions. There is, as I have repeatedly attempted to show, evident experimental cause for every detail of this behavior.

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[15] "The Behavior of Paramecium," *Journ. Comp. Neurol. and Psychol.*, 14, 1904, p. 464.

[16] "Behavior of the Lower Organisms," 1906, p. 343.

> We do not need any purpose or idea in the mind of the organism, or any "psychoid" or entelechy, to account for the change of behavior, for an adequate objective cause exists.[18]

But we need a name for such behavior; we need as it were to throw a net about it, gathering it together, so as to grasp its essential points, to the exclusion of unessential details. I have often characterized it as the selection of certain environmental conditions as a result of varied movements. For a briefer phrase, I found in common use for behavior in higher animals that is objectively similar, the expression "trial and error," so I employed this, specifying with the greatest care that it was only the similarity in objective features that I desired to bring out.[19] Later developments have shown that the designation was an unfortunate one, since readers not accustomed to distingushing the phenomena on which a concept is based from other things that may be intermingled with it in particular cases are inclined to read into it in every case all that may occur in connection with it in any case; this seems to be Bohn's method of procedure. Other authors have found no difficulty in grasping the point involved; so Driesch in his recent volumes on the "Science and Philosophy of the Organism"; and Pillsbury in his paper on "Trial and Error as a Factor in Evolution,"[20] where the concept is applied, in precisely the significance I gave it, to inanimate things. Objection to the phrase as liable to lead to misunderstanding among the uncritical I am glad to recognize as justified, so that I no longer employ it, but Bohn merely ranges himself among the uncritical.[21]

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[17] *Ibid.*, p. 343.

[18] *Ibid.*, p. 342.

[19] See the note at the end of my paper on "Trial and Error," "Contr. to the Study of the Behavior of the Lower Organisms," 1904, p. 252; "The Behavior of Paramecium," *Journ. Comp. Neurol. and Psychol.*, 14, 1904, p. 461.

[20] *Pop. Sci. Monthly*, 1906, pp. 277–282.

Bohn's inattention to my statements as to the cause of reaction of course leads him to suppose that whenever I speak of trial and error I mean that there is no experimental cause for the reactions; hence if he can show that there is experimental cause, this does away with the concept of trial and error! He therefore takes up a number of cases of reaction described by me (pp. 172–175, 187–195, etc.), shows, as I had done before, that they are due to changes in environmental conditions, and concludes that my exposition is wrong; that the application of the idea of trial and error is a mistake. His idea of trial and error is that it involves *chance,* in the sense of being undetermined.

> By the analysis that I have made above, I have been led to reject many of the pretended trials and to show that the *chance* was most often only an illusion (p. 279).

In whose mind was the illusion? My own statement has been:

> Everyone of these movements is, of course, as absolutely determined as the most orthodox tropism.[22]

Again, Bohn says: "Indeed, many of the sinuosities of the path followed by the lower animals can find an explanation in the combination of the tropisms and the phenomena of sensibility to differences" (p. 279), and in all seriousness he copies some of my figures and instructs me that the reactions shown are due to changes in the environmental conditions, just as I had shown before him. Some of the facts here are almost incredible; they throw a strange light on the author's accuracy and intelligent grasp. Thus, in the case of one of the figures copied from me (on p. 188), from which he draws on p. 191 the conclusion that I have "not taken into account the sensibility to differences,"

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[21] It is a remarkable fact that in an earlier review of my book (*Bul. Inst. Gen. Psychologique,* 1906 (?), p. 282), Bohn understood correctly my usage of this phrase, as well as various other points that he now misunderstands. It would be most interesting to know the internal history of the change by which Bohn has been brought from a position of intelligent comprehension to one of blind misconstruction of my work. It is manifestly not due to further study of my work, since, as we have seen, he now shows extraordinary lack of acquaintance with the main points I made.

[22] *Journal of Experimental Zoology,* 3, 1906, p. 452.

in the very description of the figure, that I gave, both in my *Contributions* (p. 55) and my book (p. 139), *I stated repeatedly and in detail that the cause of the reaction was the change in intensity of the light.* I even pointed out on the figure just where this cause came into operation. I added this in the explanation of the figure (though given also in full in the text), in order that no one, however careless, could miss it; but Bohn has succeeded in doing so. In an account of the reactions of a ciliate infusorian that purports to correct my own, at the moment when the animal reverses its course the author says: ''It is at this moment that I introduce the notion of sensibility to differences'' (p. 193). This introduction having been made years before[23] and the acquaintance having been intimate ever since, the ceremony is hardly worthy of the solemnity with which the author invests it.

Bohn's inability to catch the idea of ''trial and error'' as I presented it is further due to two other points: (1) To speak merely of sensibility to differences seems to me quite insufficient for characterizing the reaction; the most important point is *what the organism does.* Different organisms do very different things and the same organism in different conditions reacts differently, under the same stimulus. What the organism does is the matter to which my attention has been primarily directed. Now, in those reactions that I called ''trial and error'' the organisms perform complex movements of such a character that they subject it to many different conditions. This was the basis for the use of the word *trial,* it meant this and nothing more. (2) Bohn is content to speak of ''sensibility to differences,'' without analyzing the matter further. I carried the analysis much further, bringing out, not only the fact that the organisms react to some environmental changes and not to others, but that there is some system in this; that there is a relation between the changes to which the given organism reacts, and its own internal physiological processes; I tried to show that it reacts as a rule to changes that interfere with its physiological processes, in

[23] '' The change in the solution at this point produces the characteristic reflex ''; Jennings, 1900, *Amer. Journ. Physiol.,* 3, p. 399. '' Many sorts of changes in the environment produce a certain characteristic reflex '' etc.: Jennings, 1900, Woods Hole Biological Lectures for 1899, p. 104. '' The direct cause of the reaction is a change in the nature of the surrounding medium '': Jennings, 1901, *Amer. Journ. Physiol.,* 6, p. 35. See also quotations given on a previous page.

such a way that it leaves the region where these changes occur. It was here that I pointed out especially that for each step "an adequate objective cause exists." These relations, which Bohn neglects, are fundamental both for an understanding of the concept under discussion, and for any real understanding of behavior.

2. The same causes that prevent Bohn from understanding what I meant by "trial and error" equally prevent him from understanding my criticism of the tropism theory. He says:

> Jennings, from lack of having made the distinction between tropism and sensibility to differences, has been led to criticize, in an unjust manner, the tropisms of Loeb (p. 173). The notion of sensibility to differences has escaped Jennings; this investigator made the mistake of confounding it with that of tropisms, etc., etc. (p. 179).

Now, my reason for criticizing the application of the theory of tropisms was precisely because I *did* make the distinction Bohn speaks of. I dealt with a question of *fact*. The reactions of the organisms I studied had been commonly designated tropism (or taxis) and I used the same term until I had shown clearly what the nature of the reaction was. Then, being interested in the *facts* of behavior, I made an analysis showing that as a matter of fact the behavior did not consist of tropisms, that "the theory of tropisms is not of great service in helping us to understand the behavior of these lower organisms."[24] I can readily understand how it might be held that the theory *is* of service for certain reactions, but it is hardly worth while to attack my presentation on such mistaken ground as that I did not distinguish tropisms from reactions to variations in the environment. I have set forth in full my position on the tropisms in a recent paper,[25] so I may pass over it here. There is little in Bohn's impassioned defense of the tropisms that I could not assent to in principle; though I might object to the application to particular cases, and especially to the views which he attributes to me on the matter.

Points with which I could warmly agree are the following: He warns against "the deplorable results into which authors have been led by adopting the view according to which there would be tropisms everywhere" (p. 241). He says that "the activity of lower animals is composed of complex elements;

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[24] "The Theory of Tropisms," Pub. Carn. Inst., 16, 1904, p. 105.

[25] "The Interpretation of the Behavior of the Lower Organisms," *Science*, May 1, 1908.

tropisms, phenomena of sensibility to differences, associative phenomena'' (p. 241) (he might have added others). ''It is very possible that the nervous system and the sense organs lend their help and facilitate the processes of which the tropisms are the consequence'' (p. 124); the tropisms are variable (p. 132); they are mixed with all sorts of other activities (p. 132). ''It is often said that the tropisms are *irresistible* movements, but this applies of course only to the case where the tropisms are of an intensity sufficient to overwhelm the animal; to annihilate its other motor manifestations'' (p. 132). A similar statement could of course be made for any cause of movement whatever. Bohn's definitions of the tropisms seem on the whole such as to command assent, though I reserve certain criticisms in detail, such as the strange bringing of the *will* into the definition of an objective phenomenon (p. 117, etc.).

As to the views attributed to me, Bohn says ''for him the tropisms would be the result of an apprenticeship; of long series of trials'' (p. 137). In this and similar passages the author follows his usual plan of making a positive statement without citation of his basis for it. I am not aware of ever having expressed any such view as is here attributed to me. On the contrary, I have said that the method of trial and error ''is in complete contrast'' with the tropisms,[26] while on the same page (and elsewhere) I have said that behavior of the stereotyped character of the tropisms occurs also.

3. As remarked above, Bohn assumes that it is necessary to correct my account in detail as well as in principle. His usual method, as we have seen, is to give the same explanation that I had already given. Either a feeling that nothing good could come out of my work, or his natural habit of mind, has led him in other cases into extreme carelessness of statement. One or two examples must suffice. On page 190 he says, in discussing my account of reactions to light:

> There is a factor of which Jennings has not taken account, it is the factor of *time. A certain time is necessary* for orienting itself.

Compare the following from Jennings:

> Thus the orientation is gradual and for a certain stretch after the light has begun to act the organism is not completely oriented. With a fairly strong light however, the period of time required for complete orientation is very slight.

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[26] ''The Method of Trial and Error,'' Publ. Carn. Inst., 16, 1904, p. 50.

My entire analysis, properly understood, will be seen to largely depend on the fact that a certain time is required, and to be devoted to accounting for it. This analysis Bohn has evidently never looked into with care, since, as we have seen, he is not even aware that I attributed the reactions to variations in the intensity of light. It is well to give the devil his due; if one does it with care he may find he is not dealing with the devil at all!

Among the most remarkable feats of the author along this line is his attempt to recount and explain my description of the behavior of two amœbæ, one of which pursued and captured the other. The captor *a* after engulfing *b*, returns on its course, carrying the prey *b* (at 6, 11, 14 of Fig. 21 in my book; at 4 and 7 of Bohn's copy of the figure). Bohn says that the returning of the captor *a* is a mere recoil ("recul"), due to the *separating* of *b* from *a*; in place of *b* a glass rod would do just as well.

> The Amœba *a* quits with difficulty the contact with the solid body which one moves; when one breaks the contact suddenly the mechanical change which results from it for Amœba *a* determines the recoil of the latter.

In the description of the figure the author points out just where the "recoils" occur. Now the facts are, as I took pains to describe in my account, that this reversal of movement which Bohn calls the recoil (as at Bohn's *4, 7*) takes place *not* when the prey escapes, but after the captor has enclosed it; having captured its prey the captor returns on its course. Bohn's explanation is then quite inadmissible. In copying part of my figure Bohn unfortunately omits precisely the portion that shows the larger amœba carrying the smaller after reversal of its course (at 6 of my figure).

4 and 5. I have given a sufficient number of illustrations of the author's accuracy and grasp of my work to make it easy to conceive the value of his accusations of indeterminism, of anthropomorphism; of such criticism as "Jennings has not carried far enough the analysis of the movements of the lower organisms." (This from a writer who does not so much as know that I attributed reactions to variations in the environmental conditions!)

[27] "Reactions to Light in Ciliates and Flagellates," Publ. Carn. Inst., 16, 1904, p. 59.

The attributions of indeterminism, anthropomorphism, etc., have their source in his mistaken statement that the idea of reactions to variations in environmental conditions had escaped me; in his book he places them on that basis. With the demonstration of his mistake on that point they fall to the ground. Let us put the matter clearly once for all. Either Bohn can or he can not cite passages from my work which justify his assertions and implications that I deny determinism and that I give anthropomorphic and non-objective explanations in place of causal ones. He has not as yet made, either in his book or his numberless papers, any attempt to give such citations. If he can do so, it would be worth while, that we may see on what basis he is proceeding. If he can not, to continue to make such statements is unscientific, for they are not verifiable.

Alas! then, we find that our author does not stand the test that we hoped might set at rest the doubts as to the accuracy and trustworthiness of his scientific results in difficult fields. I have not attempted to test the remainder of his account, but I fear that similar qualities might be found there. Has the author shown, in his account of the work above analyzed, accuracy and care of the same kind that he employs in reporting what happens in the infinitely more difficult field of nature? If not, why not? And if he has—How much are his scientific results worth?

From the literary standpoint the book is one that makes interesting reading, and many of the general ideas are worthy of attention. But such confusion, inaccuracy and misstatements of fact as we have pointed out above are almost or quite sufficient to remove it from the field of science.

H. S. Jennings.

MAMMALOGY

Osgood's Revision of the Mice of the Genus Peromyscus.—The genus Peromyscus is one of the most widely distributed and most numerously represented genera of North American mammals, its range including the whole continent from the Arctic barren-grounds to Costa Rica and Panama. It is, furthermore, one of the most interesting from the viewpoints of morphology and evolution, and also historically, in as much as it typifies and illustrates the progress of North American mammalogy. It includes, as at present restricted, only the small field mice